

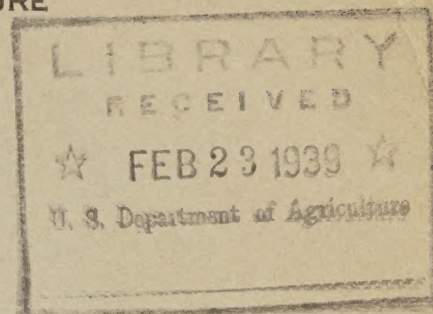
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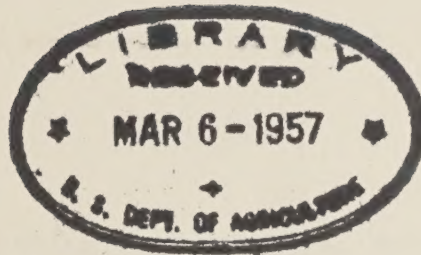


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SPECIAL LECTURES ON ECONOMICS

IN THE DEPARTMENT OF AGRICULTURE

AND THE UNIVERSITY OF CALIFORNIA

UNITED STATES DEPARTMENT OF AGRICULTURE

GRADUATE SCHOOL

Contents:

The following lectures were delivered before the students of the Graduate School in February and March 1930, and are issued in this form for present and former students of the school.

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SCIENTIFIC METHOD IN ECONOMIC RESEARCH

By Dr. E. B. Wilson, President,
Social Science Research Council.

It is a real embarrassment to me to come here announced to speak upon a topic, Economic Research, of which I have never known much, and today know less than I have at times in the past. The awkwardness is not alleviated by my appreciation of the fact that there are several in the audience who could talk about the subject better than I, and with that increased assurance and authority which comes of a wider experience and greater personal accomplishment in the field. Furthermore, the title of my lecture contains the cryptic words "Scientific Method" which slide smoothly from the tongues of many persons dealing with relatively inchoate research fields, but are apt to stick in the crop of those whose major experience is in well developed branches of science. I could, for instance, hardly imagine a group at the Bureau of Standards asking me or any other for a lecture on scientific method in physical research. Indeed, I should not be astonished to hear physicists say there was no such thing as scientific method, that there was an infinite variety of techniques, theoretical and experimental, available to the physicist from which each individual must select for a particular problem those which may be applicable to that problem, but that as to scientific method in general there was little which could be stated other than something very general with respect to the scientific attitude of mind, patient and unbiassed searching for facts, earnestness in seeking for corroboration by others wherever personal bias may have influenced the array of facts or the conclusions drawn from them, and a persistent effort to improve techniques so that facts may become more objective and precise. There would probably be some who boldly would affirm that method was for the other fellow and not for themselves, meaning by this that each individual investigator, and especially those of any real genius, pursued his own studies without conscious dominance by any notion of scientific method until he came to the somewhat unwelcome task of writing up his work for communication to others, when perforce he must consider what method of presentation would most effectively convey his findings and their justification or validity to that particular group of fellow scientists whose good opinion he would most value.

I might state as a generalization that the interest in scientific method on the part of investigators in any scientific field varies inversely as their interest in that field of science. But this statement is much too striking to be scientific; in the first place there are always exceptions and it would have to be understood as applying on the average, and in the second place there is no way known to me to evaluate quantitatively either the interest in method or the interest in the field which would justify so precise a statement as that the one interest varied inversely as the other.

I do not, however, propose entirely to cheat you of all discussion of the topic of my lecture. I will enumerate some methods.

(1) The method of definition. The choice of a suitable definition is often the touchstone of scientific advance. Scientific definitions need not be explicit, they may be implicit; they need not be quantitative, they may be first qualitative. There are numerous illustrations which could be given from the economic field, but we may all get forward faster if we take an example from physics. Let us define force. And let us go back to Newton. In his first law of motion he stated that a body at rest remained at rest and a body in motion remained in motion in a straight line at uniform velocity unless acted on by some force. Now this cleared the way. It eliminated once for all the notion that bodies stopped of themselves - they had to be stopped or started, accelerated or decelerated by applied force. There is here a qualitative and implicit notion of force which can furnish a sound basis for the quantitative definition of force as equal (or proportional) to the rate of change of motion. As a matter of fact a single definition cannot well exist by itself, it can only be part of a system of definitions - force, mass and acceleration are tied together. At any particular time in the advance of a science some new term may seem to be defined absolutely alone and the complex of definitions to which it belongs may not be perceived because this complex lies in the general background of our accepted thinking whereas the newly defined element emerges.

From such experience as I have in teaching and in research, particularly in emergent fields, I should say that most pupils and colleagues were so thoroughly oblivious to the necessity for and significance of definitions in science as to be almost impatient of them. In this they are far from scientific. Science is a congeries of definitions. We are seeking agreement in science and one of the most effective ways of seeking it is by agreement on the meaning of our words. Take such words as "wealth", "income", and others which are basic in economic research. How can one expect agreement between two persons who are tacitly using different though perhaps unformulated definitions for these or similar words? Of course, I do not wish to over-emphasize the necessity for precision in definitions - there is no such thing as absolute precision apart from the construction of complete logical systems such as mathematicians have constructed for arithmetic, geometry and algebra of several types. We have at any stage of science to deal with a relative precision of definition. And it may not be amiss for me to point out that the absolute precision of the completely defined mathematical discipline exists only for that intellectual discipline and breaks down immediately when that system of thought is applied to nature, by virtue of the fact that there do not exist in nature any objects precisely corresponding to the exact intellectual concepts precisely defined.

Let us consider the question of "level", "rolling", or "hilly" agricultural land. It would doubtless be possible to construct mathematical definitions of level, rolling, and hilly in such form that a surveyor equipped with the proper instruments could classify land under these three categories with great scientific precision, by which we mean that specifications for the classification could be so drawn as to make certain that different surveyors equipped with similar instruments would make the same classifications with only the fewest disagreements in certain very doubtful intermediate cases. Such a precise definition with its accompanying exact classification would be costly in time and in money and thus of

limited practical utility. Very little land could be so classified. On the other hand, it would be possible to set up rough and ready descriptive definitions of level, rolling, and hilly intended to be used for classifications by mere inspection which would be utilizable at comparatively slight expense in money and time. Now different observers would undoubtedly differ considerably in their allotments of parcels of an intermediate character to one or the other of the adjacent categories, and in this allotment there might be bias, so that even on the average the classifications by different observers would have different percentages of the three sorts. The reliability of the method would certainly be less, its utility, even its scientific utility, might be far greater because of its greater feasibility.

The essential element in precision is not so much absolute precision as a sound estimate of what degree of precision has been attained. It is really reliability in the sense of reproducibility or agreement which is the scientific item and if the reliability is sufficient for the purposes in hand and if the conclusions drawn from the work limit themselves to what is justified by the reliability attained, that is all we can really ask. Thus two treatises on economics neither of which explicitly defines wealth or some other term but both of which develop their analysis upon tolerably similar implicit definitions may well not come into actual conflict even though if each pushed its analysis far enough they could hardly fail to come into some conflict resolvable only through rendering the definitions more precise. Much medical work, particularly of the clinical variety, could not proceed at all if a high degree of precision were required. So to define broncho-pneumonia or dementia precox as to result in a high degree of reliability in diagnosis may actually be impossible in the present state of our technique. I do not mean that definitions could not be given but that such definitions would not be those of what should be called broncho-pneumonia or dementia precox - they would be artificial and illusory definitions. What we need at the moment is not such definitions so much as a recognition by scientific clinicians of their need for the concept of reliability, their need to have a feeling for what the true precision is; because it is only upon the basis of such recognition that one is likely fruitfully to improve the definitions. The situation may well be the same in respect to a large number of economic concepts.

(2) The method of mathematical analysis. Beginning rather clearly with Cournot, there has been a great development of mathematical analysis in economics. I do not refer to arithmetically quantitative analysis but to the type of mathematics which is logical and qualitative in the sense in which J. C. Cobb has discriminated between qualitative and quantitative (Econ. Jour. England, March 1928, p. 72). Now, as we were speaking of definitions, it may be well for me to point out that the adjective quantitative is perhaps used in a different sense by some economists from that implied here. Some might consider Pareto's work almost wholly quantitative because of his insistence on sharply defined concepts and his use of mathematical equations and transformations, including differential equations and integrals, etc. The notion of marginal utility is in a sense quantitative, and so is the concept of the curve of indifference which Pareto was inclined to make basic. Yet this must be said: That his whole treatise goes very little way, and the whole spirit of it is not conceived

in the manner to go far toward the determination of the numerical values which should be inserted in the place of the variables in his equations to arrive at a check on the analysis. Indeed, as I conceive it, he was not interested greatly in that sort of check; his interest lay in the logical analysis of the sorts of situations which could arise. In a sense that is qualitative rather than quantitative, and at any rate I want to make it clear that I used qualitative to cover what others might prefer to call quantitative.

Mathematical analysis depends on conventions. If the convention is adopted that the price of a commodity depends on the amount of that commodity one has the system of Walras, and the mathematics is relatively simple. If one allows that the price of each commodity may depend on the amounts of all commodities one has a more complicated convention leading to more complicated analysis. This latter form of analysis is surely so highly involved that one could scarcely carry it on without the mathematical method. If then commodities are really so interlocked that many important economic events can only be understood by taking into consideration the possibilities that arise through the interlocking and would not arise if there were no interlocking, there is no escape from a consideration of the complex situation and no escape from the use of a good deal of mathematical thinking with no small amount of mathematical symbolism. It is perhaps true that many serious students of economics are today being developed without any considerably training in mathematics. Maybe they can so choose their work throughout their life as to avoid problems which require a knowledge of mathematics, and maybe they will learn their mathematics in later life as W. G. Sumner did, according to his personal testimony to me.

I may say that it has been maintained that man's essential characteristic lies in his effort to cover not space alone but duration of time (Korzybski). It may be that prices do not depend merely on quantities of commodities in existence but on those and quantities that may come into existence. In recent months we have been told that what was important in the price of a stock was what it was going to sell for. Very likely. There is certainly speculative adjustment of prices. If that complex of phenomena which arise from man's time-binding propensities is of relatively little economic significance, it may be neglected, but if it be of considerable significance, economists will arise to treat it and they will probably be forced to use much more complicated mathematical thinking than Pareto used - there may be integral equations and integro-differential equations. It is not impossible that they cannot even state adequately the essential logical interconnections of the conceptual situation which arises in time-binding economics and cannot formulate the proper conceptual complex without appeal to a type of mathematics so advanced as to make it necessary for some mathematicians themselves to become economists. Indeed, Pareto started as an engineer, I believe a railroad engineer; Irving Fisher as a mathematician, and so more recently Roos. The mathematical method is not yet through in economics; indeed, it may be just beginning, but it is likely largely to remain the work of a small fraction of students of economics.

(3) The statistical method. Although the statistical method is highly valuable in dealing with classifications of data not yet reduced to quantitative form, presumably the chief interest of the economist in

statistics is quantitative. There is of course no such thing as the statistical method. Statistics is a complex of techniques. Take, for example, the sampling technique, the sampling error, and the question of the fairness of a sample - this whole group of ideas which is of great importance in social studies and in experimental biology may be of little interest to the economist working in some such line as the theory of the business cycle. If we allow an average duration of 40 months, we have in 40 years some twelve cycles. Of what can those twelve be regarded as a sample? Of course they may be regarded as a sample of the universe defined by themselves, which does not get us ahead very far. Indeed 40 years takes us back to 1890, before Bryan, before the U. S. Steel Corporation, before the war, and before the "New Era" of 1929. In many ways the economic situation has so changed that there may be no sense in asking the question as to what universe the sample comes from. Yet how talk of sampling errors without samples or of samples without their appropriate universe? I do not wish to imply that there are not opportunities to use sampling methods in economics; there are many such, but on the other hand we have many problems where the opportunity is lacking.

Then there is the regression technique. What a regression equation does is to give us the mean value of the dependent variable in terms of assumed values of the independent variables. A regression equation may not be solved for one of the independent variables to get the new regression equation. This is a limitation on the use of regression equations for mathematical analysis. Indeed the mathematical theory of economics assumes that the variables occurring in the equations are like those occurring in thermodynamics or other fields to which analysis is applied in that they have values, not that all the independent variables have values and the dependent variables only a mean value. This is a real difficulty in coordinating the mathematical and the statistical methods so that the latter furnishes concrete and practical illustrations of the theoretical developments of the former. It is a difficulty which must persist so long as inherent variability due to lack of complete control is present. Thus one of the chief techniques of the mathematical economics is that of counting the number of variables and the number of equations to verify that these numbers are equal and that the problem is therefore determinate, whereas statistical procedures are applied in just those cases in which there still remains indetermination, that is, where the number of equations is fewer than the number of variables and it becomes necessary to treat the dependent variable as an assemblage of values and take the mean or median or some other special value as representing the group of undetermined values.

The analysis of time series is a technique which has remained rather specially that of economic statistics. It is employed very slightly in other applications of the statistical method. Time series are treacherous, but I do not see how their treatment can be ignored. Also it is not easy to see how the treatment can be made scientifically satisfactory; in the nature of the case we may have to be content with exploratory methods. Of course we can apply the method of the periodogram and the resolution into trigonometric series. Such analyses work well on observations of the magnitudes of some classes of variable stars for which there are presumptively true periodicities in the background of the observed time series. When however the method is applied to meteorological or economic phenomena somewhat bizarre results are often obtained, results which do not always,

perhaps do not usually, serve as a satisfactory basis either for forecasting the future or for representing the past except for that limited portion of it which has been used in the analysis. Indeed at the present time I should say that the evidence that there are cycles does not justify one in assuming that there are periods in the sense required for sound periodogram analysis.

One of the chief aids to the advance of scientific knowledge is the existence of artificial or natural repetition in the sequence of phenomena. The extreme regularity of the real motion of the planets and the moon enabled the ancients very considerably to unsnarl the obvious irregularities of the apparent motions and to construct calendars of a high degree of prevision, to forecast eclipses with considerable success, and in general to develop the science of astronomy. The periods involved were not all short compared with human life, but the basic regularity allowed observations to accumulate. In the laboratory we may repeat experiments as often as we please without serious danger that conditions have seriously changed, - indeed the first and basic type of control is repetition. In the economic field the situation is quite different; so far as is yet known we do not have highly regular basic periods nor do we have conditions that may be repeated without serious changes, and we are very much handicapped thereby in scientific advance. It is true that the rapidity of change in conditions varies greatly at different times. The younger generation of economists have seen tremendous apparent changes within their own relatively short lives. It may be that a prolonged period of relative stagnation might help toward the discovery of some economic laws by making it easier to ignore the trees and concentrate on the forest; such a period would however have compensating disadvantages through the temptation to a feeling of security with its inevitable resulting dogmatism.

After all, it is perhaps remarkable not that so little but that so much has been accomplished in selecting from the mixed, complex, and variable economic field some items which have been reduced to method, by definition, by mathematics and by statistics sufficiently to merit the application of the term economic science. Whether the proportion of economic instruction in our universities which can be rated as scientific is today larger than it was a generation ago may be difficult to determine - there is much more economics taught, much more that is descriptive, illusory and unscientific, also much more that is sound and scientific. In the governmental, banking and business activities of our country there is also an increase in the amount of economics in use and very likely an increase in the proportion of scientific economics. The scientific economist may look at the future with caution as to what economic events may occur, but he may look at it also with confidence that the scientific method will continue to conquer new worlds for him as for his scientific confreres in other scientific fields.

EVALUATING INSTITUTIONS AS A FACTOR IN ECONOMIC CHANGE

By Prof. John R. Commons, University of Wisconsin

Before we can evaluate institutions we must tell what we mean by them. By institution I mean concerted action in control of individual action. As such, an institution is not something new to economists; it is something that they have always considered. Adam Smith opposed institutions, on the ground that they interfered with liberty, which is true enough -- they do interfere -- they even control. Smith opposed not only guilds, corporations and mercantilism, he opposed what would now be called rotary clubs and even telephone directories, because they enabled competitors to get together and act in concert.

Certain recent economists, under the impression that they had become institutional, have seemed to mean by an institution merely a class of people habitually acting alike, as when they move their goods to a market for the purpose of sale or exchange. But this is only an analogy to the physical sciences. The waves of the ocean act alike. Adam Smith and all his following would agree to this kind of similarity, for they assumed that everybody in business acted alike for his own self-interest. You can build a science of the universe only because you assume that all electricity acts like the little piece of it in your laboratory. But that is not an institution. It is physics. Physical economists omit the essential of an institution, the concerted compulsion on individuals to act alike. In so far as individuals merely act alike, that fact may be the result of similarity of instincts derived from heredity, or similarity of habits developed in a common environment. But only when individuals are compelled by concerted action of others to act alike does their similarity of action become an institution.

For institutions arise just because people do not act alike. They arise from conflict of interests, and that is the reason why Adam Smith could not stand for them. His presupposition of self-interest was his avowed divine harmony of interests. But when there is conflict of interests then there must be concerted action to compel conformity of individuals to the activities of others.

This does not mean that every individual is conscious of compulsion. He willingly acquiesces because concerted action brings to him more liberty than it suppresses, depending on the way in which it is conducted. Indeed the only liberty that we know has come about through compulsion of those who would interfere with it. Compulsion of others creates liberty for self. This has been recognized by economists in their theory of the marginal man. He is the man whose destructive, or unfair, or deceitful, or irresponsible competition compels the others to come down to his level. But concerted action restrains the liberty of the marginal man in the interest of creating more liberty for those who would like to be super-marginal.

Thus the theory of the marginal man is quite different from the theory of marginal utility, or marginal commodity, or marginal product. Those theories were mechanistic, based on analogies to physics, or on the so-called natural order or natural harmony of the universe, or on the supposed tendency of nature's forces towards equilibrium. They reduced human beings to what Veblen called "globules of desire". But the marginal man is a person; concerted action is a consensus of persons; and concerted action is an institution which creates duties, liberties and rights.

Indeed, the theory of concerted action in control of the marginal man is recent. It is based on conflict of interests and therefore had no place in the earlier theories based on preordained harmony of individual interests. But conflict of interests, in contrast to Adam Smith and his followers, is the basis of all modern theories of so-called stabilization, orderly marketing, live-and-let-live, and so on. In fact it is also the real basis of a theory of credit. A debt is not a commodity, the so-called circulating capital, as the classical economists thought. A debt is a negotiable institution, made so by concerted action. Self-interest might lead the debtor to change his mind and refuse to pay, but concerted action makes him pay, even against his apparent self-interest. A debt is an institution in action, and it greatly enlarges the liberty of those willing to pay and willing to lend, by giving them greater confidence in each other.

It is on account of these physical instead of institutional concepts that those historical economists who followed Karl Marx's technological evolution of the means of production (under which they included transportation and marketing), as well as those who followed the marginal utility theories, usually overlooked the essential part of the primitive markets, the pie poudre courts, which were the impromptu courts of the fairs conducted by the merchants themselves, out of which many of the basic principles of our common law originated. Economists looked upon markets as a similarity of self-interest, but those early courts were the concerted action of the assembled merchants to compel conformity wherever conflict of interests violated their joint rules designed to insure an open, free and equal market. Thus the institution of markets is the substitute for Adam Smith's instinct of "truck and barter". The institution is the compulsion of concerted action upon the transactions of individuals upon the market. The successors of the pie poudre courts are the stock exchanges, produce exchanges, commercial and labor arbitration, cooperative marketing, which in innumerable ways lay down the rules which participants must obey if they would make a living by selling and buying on the markets. And we can now see some of the rules and practices of these modern economic institutions coming over into the new common law of business, by the decisions of courts, just as five hundred years ago the courts adopted and enforced what they thought were the reasonable practices and rules of the primitive courts of the peddler-merchants. Economic institutions are the progenitors of political institutions.

Hence an institution is a law-making association in so far as its action has power to enforce rules upon individuals. Whatever kind of behavior is imposed on individuals by concerted action is institutional behavior. The fact that a worker in the painters' union works eight hours a

day is institutional behavior -- or the fact that a Stock Exchange broker charges a fixed rate of commission is an institutional form of behavior.

Thus an institution is not the older analogy of a framework like a building, nor of a "form" which shapes its content. Its content is its form. The people who make its rules also are bound by them. The institution is not an imposition, like a bridle on a horse. It is the way people act in concert.

There are many kinds of institutions such as labor, finance, debt, contract, etc. All of these institutions, in whatever way they may be classified, do, however, have fundamentally similar aspects. While all of these aspects are inseparable in the institutional behavior of individuals, we may classify them for purposes of analysis. For this purpose we may examine an institution from the viewpoint of its (1) activities, (2) products, (3) working rules and (4) sanctions.

Manufacturing, for instance, is an institutional activity. Automobiles, steel, etc., are the products of this activity. The law, trade practices and agreements are its working rules, and the fines, imprisonment, expulsion from trade organization and financial loss are some of its sanctions.

Here is the difference between Competition and Custom and between Custom and Going Concerns. It has been usual, since Jeremy Bentham's attack on Blackstone in 1776, for economists to set competition over against custom and to follow Sir Henry Maine in his assertion that we have passed out of the age of Custom into the age of Freedom of Contract. But if the earmark of a custom is its compulsion on individuals, then contract itself is also a custom. He who refuses to bind himself by contracts, as others do, cannot continue in business. Contracts are customary and therefore compulsory. And that which in England and America is known as a going concern is merely a custom of organizing concerted action. He who cannot join in concerted action is now the sub-marginal man who cannot prosper. Already it is estimated that eighty-eight per cent of the manufacturing of the country is conducted by corporations.¹ These are the going concerns of modern industry.

Customs and concerns are the two classes of institutions. The difference between them is in the mode of selecting individuals who enforce their rules. No organized institution, except in time of war, insurrection, strikes, lockouts, calls out all of its members to enforce its sanctions. The sanctions are enforced by representative individuals. In the case of custom the enforcement of sanctions falls to chance individuals with whom the particular individual happens to come in contact. In the case of going concerns the sanctions are enforced by such duly constituted individuals, as the court, sheriff, foreman, or agent, who are authorized and ready to act on behalf of the concern. In the case of custom these chance individuals are the customers and competitors who enforce the rules accepted as customary. The modern custom of purchasing commodities and

1 National Industrial Conference Board, Federal Corporation Income Tax, 172 (1928).

paying debts by means of bank checks is not a mere habit, or instinct, or convenience. It is compulsory on individuals, for whoever persistently refuses to accept and use bank checks, although they are not legal tender, cannot continue or even get into business. Thus custom is not the contradictory of competition. Competition is a means of enforcing custom.

But in a going concern, such as a business organization, a labor union, a farmers' cooperative, and the State itself, the rules are enforced by constituted authorities and agents, selected for the purpose and acting with the support, if necessary, of the potential concerted action of all the members.

It is by these two methods of concerted action -- custom and going concerns -- that a derived institution, the rights of property, is created. Usually it is assumed that only the political institution, with its legislatures, courts and executives, creates and determines what shall be the rights of property. But economic institutions do the same, and they do it even before the political institution copies it. The difference lies in the kind of sanctions which concerted action employs to enforce obedience. The political institution uses the sanction of physical force. The economic institution uses the sanction of economic power. The loss of a job, the exclusion from a market, the discriminations and inequalities in bargaining transactions, and other losses or gains beyond individual control which always accompany concerted economic action, are usually more powerful, prompt, and decisive than the uncertain and delayed physical sanctions of the courts. A man can be sold out in ten minutes according to the rules laid down by the brokers on the stock exchange. Or a laborer can lose his job in one minute and not get legal damages in a life-time. The stock exchange is an economic institution. Its by-laws create rights of property beyond what the courts create. So with other economic institutions. They change with economic sanctions, and the courts follow with physical sanctions.

Indeed, even the institution of the state itself, although it is a separate factor in bringing about economic changes, is subordinate to, and does the bidding of, economic concerted action. A political party is a modern economic institution designed to get control of the political institution. Its activity is the collection of enough votes to control the legislature and the selection of judges and executives, on the promise to the voters of distributing thereby to individuals, corporations and classes a greater share of the limited wealth of the country. A protective tariff is the concerted activity of log-rolling and hold-up, whereby the physical power of the state, in raising or lowering prices and thereby redistributing quantities of wealth, is rationed out to different economic interests according to the demands which each is able, by concerted action, to enforce on all the others. So it is with other forms of taxation. While economists rely on the benefit principle, the cost principle, the ability principle -- which are really not principles but only justifications -- the wealth of the country is actually rationed by such concerted log-rolling as farmers, manufacturers, laborers, political lobbyists, may be able to exert for and against each other in getting control of the physical force of the state.

This is again an institutional process of the distribution of wealth and opportunity, and must be distinguished from the logical theories deduced from the assumptions of free competition, harmony of interests and laissez-faire, on which economic science was built by Quesnay, Adam Smith and Ricardo, and on which the marginal utility theories continued to build down to the end of the Nineteenth Century. The founders of the science lived in what is rightly called the Age of Reason, which assumed that man was a rational being who would act according to reason if only he were enlightened. It was Malthus, in his great essay of 1798, who first attempted to disillusion the economists. Man is not a rational being -- he is a being of passion, stupidity and justification, who does exactly the opposite of what either his reason or universal reason admonishes, and then justifies it. Hence misery, vice, poverty, overpopulation, economic conflicts and wars rise up to confound all the rational propositions of the Age of Reason and its theories of divine or natural harmony. Indeed, what was called Reason in those deductive propositions turned out to be largely justifications of what they wanted to do or wanted other people to do.

So it is that institutional economics, the economics of concerted action, returns to Malthus. Masses of men are not rational. They are institutional. Institutional economics is the economics of passion, stupidity, justification or crimination, and concerted action. These can be investigated scientifically, like anything else, and their investigation is the way in which institutions can be evaluated as factors in economic change.

What is needed, however, is a new logic, a new methodology, even a new terminology. Physical science is being revolutionized from the time when economists took, as their model, Newton's harmony of the spheres, to the time when radio-activity is confounding our common sense of space and time into infinite relativity. Economists can no longer take other sciences as their model and then reason by analogy from the universe. Even the physicists are getting scared when they find their science resolved into nothing but pure mathematics. The economist is acquiring the same anxiety at the hands of the mathematicians. We are being enticed into pure number. But we have to do something about passion, stupidity, ignorance, concerted action. Ours is in fact a science of aggression, resistance, justification and crimination. This is institutional economics.

I know of no method of evaluating institutions except participation in them. This is the economists' laboratory method. Lindeman, Follett and the Lynds have shown us somewhat the logic of this method.¹ The participant learns by experience how to evaluate the institutional process. If he works within a labor union or a farmers' cooperative, or a legislature, or a business concern, he knows what I mean by an institution.

The capitalists have learned by participation how to value institutions. They do it by dictatorship of the institution. It used to be thought that a corporation was a voluntary association of stockholders joined together cooperatively for the management of the concern. They were

1 Lindeman, E. C., *Social Discovery* (1924); Follett, Mary P., *Creative Experience* (1924); Lynd and Lynd, *Middletown* (1929).

the so-called entrepreneur. But economists have difficulty in finding that entrepreneur. He has become an institution. This is because the State has injected limited liability into the stock holders' responsibilities. Now we know that the directors are often dummies and that an inside syndicate, holding by proxies a majority of the common stock when needed, names the dummies and dictates their policies. And the value of all the common stock, which controls the concern, has, in some cases, been reduced to a very small proportion of the total going value of the concern. Then, if the company is large enough, it dictates to the whole trade what shall be the prices, quantities, business practices and territories which each competitor shall observe on the penalty of cutting prices and resulting bankruptcy. This is the other process of rationing benefits and burdens -- not the log-rolling and hold-ups within the legislature, but the dictatorship of insiders. It is not price-fixing, which would be contrary to law -- it is price-terrorism. The outsiders, such as investors, stockholders, laborers, customers, competitors, are all obedient to the commands of their unseen rulers. They act on propaganda, emotions, stupidity, fear, justification, crimination, like most concerted action.

By the method of participation I do not mean that every economist should participate in every institution. He learns from others and they learn from him, as in all sciences. For science no longer is the lone investigator. It is the body of scientists. What the body of scientists does, distinguished from the practitioners, is to discover, if possible, the universal principles which are fundamental, and then coordinate them into the relations of the parts to the whole which every science must do if it be scientific. The economist does this by his principle of limiting and complementary factors, and it is the discovery of this part-whole relationship that is, in fact, the very process of evaluating institutions. The difficulty with the early economists was that they were satisfied with investigating the parts, such as marginal productivity or diminishing utility, or supply and demand, and did not investigate all of the parts which constitute the whole. Prices, for example, are mere abstract statistics. Back of the prices are price agreements, and price agreements are institutional activities.

I take it the reason why Nineteenth Century economists evaluated prices and not institutions was because they did not appreciate what it was that was actually exchanged upon the markets. They thought it was commodities and services, but it actually is rights of property. When I hand to you a book and you hand to me a dollar, they thought that was an exchange. But that was only a part of the whole. It was only a labor process of physical delivery. What was actually exchanged, as lawyers know, was two legal ownerships. This is not a labor process -- it is an institutional process, for no individual can of his own power transfer the ownership of anything. He can only indicate his wish to do so. Only the state, by its rules of law, transfers the ownership, and those who finally decide which is the owner and how much he owns, if disputes arise, are the constituted courts. Then all other individuals follow the precedent and fall in line with what it is expected the courts will do in similar cases.

This is simple enough. It is the common law, based on precedent. But it was not necessary for economists to distinguish the two parts of the transaction in those early days when the seller carried his goods to market

and physically delivered them. And the economist does not now distinguish them when he persists in basing his theories on the physical facts of primitive barter or exchange, or the psychological facts of pain and pleasure. But the distinction is now apparent and insistent, when a signature in New York and a wire or wireless gives commands to agents and laborers to manufacture, transport and deliver commodities at even the remote ends of the earth. The prices are made where ownership is transferred, and price is the price of getting legal control which alone makes possible the subsequent physical control. This is institutional economics. It is the transactions and commands of property owners enforced by custom, laws, treaties, armies and navies. It is the biggest factor in economic concerted action, and must be evaluated as the biggest factor in the world economic changes that are going on. All political institutions, on a world-wide scale, are more or less subservient to it. They execute the orders of property owners.

This could not be done without concerted action of property-owners, and their concerted action could not be effective were it not that the State, as above suggested, has created a new right of property, unknown to the early economists, the universal right to incorporate with limited liability. This universal right began to be enacted by law in the decade of the 1850s in England and America, and has been extended by legislation and judicial decision until it reaches the modern form of holding companies. It was not until that decade, eighty years ago, that Capitalism really began. Instead of abolishing corporations, as Adam Smith and the anti-monopolists proposed, the State universalized them. Prior to 1850 the right of association was created only by a special act of the legislature and required lobbies and a political party to get it. It was a special privilege and therefore monopolistic. Now it is universal and informal, because all that is needed is to file articles of incorporation with the Secretary of State. And these articles are self-created, so that, if the right state is picked out, like New Jersey or Delaware, the incorporators can give to themselves all of the powers that a majority of the Supreme Court will permit. And the Court itself has gradually enlarged its permission, contrary to the anti-trust laws and culminating in the year 1920, when the Court decided against the Government in the Dissolution Suit against the United States Steel Corporation, the greatest of the then holding companies.¹ Now the right of association with limited liability is unlimited except as the Supreme Court sees fit to limit it. Not even Congress can limit it, much less a State legislature, except as the Supreme Court permits. Hence world-wide economic institutions rise out of America, superior even to political institutions, depending on the extent to which, by practical politics, they can maintain a favorable majority in the Supreme Court.

For the Supreme Court has in its hands the exercise of the two powers of sovereignty that create, revise or enlarge the rights of property - the mandatory and the injunctive power. The one is the power to command what individuals and associations must do. The other is the power to command what they must not do. They must pay their debts. They must not interfere with other persons. It is these commands that constitute rights of property.

1 U. S. vs. U. S. Steel Corporation, 251 U. S. 417 (1920).

These powers of the courts reach even the legislatures and executives as well as individuals. If the legislature must not interfere with a holding company then the concern is at liberty to do as it pleases within the limits of legislative non-interference laid down by the court. The Court holds that 7.44% on the present value of the property of a street railway company, measured as cost of reproduction less depreciation, which is 50% above pre-war value, is a property-right which cannot be reduced by State regulation of prices charged for transportation. On the other hand, in the agricultural industry of the country, if the farmer's manual labor were estimated at the level of street-car wages there would be left practically no rent, no interest and no profits on a real estate value scarcely 15% above pre-war level.¹ Back of the Supreme Court is the organized acquiescence of a nation which makes its mandates and injunctions effective. Thus do institutions become a factor in economic change.

The tendency of these institutions towards economic dictatorship seems to be inevitable, if we go back to Malthus and find, not an age of reason, but an age of passion, stupidity, and justification. With this Malthusian fact about mankind, it is necessary to restrain and command individuals by concerted action of some kind. In economics these restraints and commands are the rights of property. They are the law-and-order which economists have always presupposed as essential to their theories of commodity prices, and if law-and-order end in economic and political dictatorships then economists must develop also a theory of institutional valuation.

They seem inevitable, also, because increasingly the best way in which people can invest their savings is in the stocks and bonds of corporations. Formerly the farmers put their savings into their own buildings and improvements which they could personally control, or loaned on mortgages. Now the proportion of farmers to the total population is much reduced; the proportion of corporations is greatly increased. They do probably nine-tenths of the manufacturing of commodities, and a wider opportunity for farmers and all others, if they have savings, is to put them into banks, investment companies, stocks and bonds. They do not lend to their neighbors. They buy stocks and bonds of business institutions. Hence, willingly or not, the savings from profits, wages, rents and interest go into going concerns which the owners cannot control, and so make more inevitable the spread of economic dictatorship.

Of course economic institutions change in course of time, and we can trace historically their origin, rise, endurance and decay, by merely tracing the rise and fall of concerted action. We can watch institutions change if we watch concerted action. They change from loose to organized, from incipient to enduring, from ineffective to effective, from spasmodic to stable. When they are loose and incipient we give to them the name of Custom, but when they are organized, effective and enduring, we give to them the name of Going Concerns. When they stop going they are bankrupt

¹ Calculations based on estimates of National Bureau of Economic Research, in Recent Economic Changes, vol. 2, pp. 587, 781, and preceding comparisons of rural and urban wages in earlier volumes.

or absorbed. Political institutions have similar history until they arrive at a monopoly of physical force by prohibiting private violence.

Here is where the evaluating of institutions rises into a new importance for economic science. There is an accepted common-place that power brings responsibility. And investigation shows that these economic dictatorships recognize, at times, a greater responsibility to the public than do the little scattered competitors who are free to seek their own diminutive private interest. And, in turn, it is possible that these concerns can be held by law to a greater responsibility simply because they do not have the votes, but the small proprietors cannot be held to public responsibilities because they do have the votes. We can even point to the dates when this responsibility of dominating concerns began to be felt or enforced. We have seen it in our own day in the case of railroads and public utilities. The Bank of England, a private institution, first acknowledged its public responsibility to maintain the gold reserve in the year 1857, even at the loss of profit. The Federal Reserve System began to maintain stability in 1923, at the possible loss of immediate profit. The Steel Corporation in 1908 began to acknowledge its responsibility to customers and competitors. Others have followed. Whether this responsibility is adequately recognized or enforced is one of the big problems in the valuation of institutions.

The Malthusian theory went further than I have indicated in furnishing the foundations for valuation of institutions. Malthus introduced Scarcity into economic theory where hitherto the theories, from John Locke to Adam Smith, had been theories of divine benevolence and earthly abundance. David Hume alone had been skeptical. If there were abundance of everything, he said, so that an individual when deprived of something he wanted could merely reach out and get its duplicate equally good, there would be no private property, no need of justice, no compulsion on individuals to share with others or to avoid interference with others, and therefore, we add, no concerted action and no institutions. But wherever there is scarcity, said Hume, then all of these practices would be necessary.¹ Scarcity enslaves individuals, but institutions can make them free.

Malthus reduced Hume's speculative philosophy to economic reality. It was the passions and stupidity of man that brought on overpopulation and scarcity of food. Malthus was partly mistaken. The Nineteenth Century brought in amazing mechanical inventions, long-distance transportation, the opening up of new food resources, and the century seemed to be, for our Western civilization at least, one of increasing abundance instead of increasing scarcity.

But the Twentieth Century opened with the world occupied and reduced to private property. A world-war re-apportioned it. Agriculture became depressed in the older countries of Europe and then depressed in the new countries. Food prices did not rise -- they fell. It was now discovered, after the war, that, not food was scarce, but purchasing power was scarce. Purchasing power is an institution. Food is a commodity. If the world's purchasing power is scarce then it is suicidal to cut prices in expectation

¹ Hume's Philosophical Works, 2:188 (ed. by Green and Grose 1898).

thereby to capture more of that purchasing power. The world's limited supply of purchasing power must therefore be rationed. Hence cutting prices -- Adam Smith's notion of divine harmony -- is not only suicidal, it becomes unethical to pull customers away from competitors by cutting prices. This post-war theory would have astonished Smith, Ricardo and the marginal utilitists. And its accompanying theory of scarcity of labor would have astonished Malthus. Labor also, in America, is scarce instead of food. Hence labor also must be rationed and it becomes unethical to steal a competitor's labor by raising wages.

How then can this rationing be substituted for freedom of individual bargaining? The ethical appeal of rotary clubs, of trade associations and the modern so-called "institutes" is only an effort to establish a loose custom. Only the concerted action of competitors can command obedience of the price-cutter and wage-raiser. Thus the Malthusian scarcity is transferred from food to purchasing power and becomes the compelling force that drives competitors into concerted action in order to bring economic sanctions to bear on recalcitrant individuals. This is institutional economics, and is the increasing value of economic institutions as the means of controlling destructive competition. Like all institutions its justification rests on the appeal to ethics -- the ethics of stabilization, orderly marketing, live-and-let-live, fair competition, and the sharing, instead of fighting for, the world's limited purchasing power or limited labor power.

I would not say that this practice of rationing by concerted action is rational. It may be only the economics of aggression, resistance and justification. Yet it does not need to be rational if it is inevitable. Nevertheless, to pass judgment upon it is to evaluate institutions.

Of course it is possible that opposing institutions may rise up to contest this world-wide rationing of purchasing power. Organized labor in the clothing industry raises its wages to 90 cents per hour, where similar but unorganized labor gets one-half as much, and unorganized farmers get scarcely one-third as much. After forty years of effort the laborers get immigration restriction which raises their wages more than the cost of living has risen. Other comparisons might be investigated.

This ability, however, to make use of the institutional process depends on the Malthusian estimate of human nature and on the willingness of courts to permit it. If the opposing institutions have for their foundations only passion, stupidity and crimination, then they are only spasmodic. And even if they go further and attain an element of reason, then they may collapse because a court enjoins them. A Colorado court prohibited the sugar-beet-growers from organizing a cooperative that could bargain by concerted action with the owners of the one great sugar refinery which was their sole purchaser; but the Wisconsin court authorized and enforced the similar arrangement of the Tobacco Pool.¹

1 Mountain State Beet Growers' Marketing Assn. vs. Monroe, 269 Pac. 886 (1928) (Colorado); Northern Wisconsin Tobacco Pool vs. Bekkedal, 182 Wis. 57 (1923).

Thus the political crimination of opposing economic institutions by the courts is just as destructive of concerted action as the passions and stupidity of the membership. For these reasons the Federal government has now come to the aid of these same farmers' cooperatives. Similar considerations hold true of labor unions in opposing employers, or of retailers in opposing chain stores, or of motion-picture exhibitors opposing motion-picture producers, and so on. Passion, stupidity, judicial opinion, make them ineffective against the economic dictatorship of nation-wide concerted action of business institutions. Even if the State, through its legislatures and courts, aids and authorizes them, the two problems still remain, Can they act in concert, and Who will control the State?

The difficulty in evaluating institutions arises from the mental process of valuation itself. All such valuation has a three-fold object to be valued -- the purpose, the instrument, and the method of controlling the instrument. The purpose may be good or bad; the instrument may be effective or ineffective; the control of the instrument may be wise or stupid, honest or corrupt. The three are inseparable. The instrument, in this case, is concerted action; the purpose is the production and distribution of wealth; the control of the instrument itself is its administration by those in authority.

The most powerful of the new economic institutions is doubtless the Federal Reserve System. The underlying reason for its existence is recognition of the change in modern economy from the Malthusian scarcity of natural resources to scarcity or abundance of purchasing power. The Reserve System was created with practically no limits placed on the discretion of those who administer it. It brought on a world-wide inflation in 1919, a deflation in 1921, and an approach towards stabilization from 1923 to 1929. The System is the concerted action of 10,000 banks which furnish the bulk of the nation's purchasing power. It is made effective by pooling their gold reserves, by rationing reserve credit to the banks through open market and discount policies, and by making rules for the use of that credit. Such concerted action was inevitable, and even too long delayed. Nearly every nation in the world has resorted to it. Its purpose is vaguely stated as the "accommodation" of commerce and industry. What that means is left to the judgment of those who control the system, but it is our greatest instrument for the distribution of property-rights in their active sense of bargaining and pricing. To what extent it is efficient, depends on what purpose its efficiency is expected to accomplish. Hence to evaluate that institution is to pass the threefold judgment on what should be its purpose, what is its efficiency, and what are the practices of those who control it. So with any institution, from a farmers' cooperative or a labor union to the Standard Oil and a nation. Its purpose, its efficiency, its method of control, must be evaluated as the whole of a going concern.

This is not something new. It is what every investigator of any form of concerted action is already doing. The economic inquiry turns on these three factors of an institution. Is its purpose in the interest of the public or is its purpose solely greed? Has it reached a sense of responsibility that goes with power, or is it animated only by the self-interest which Adam Smith thought was enough on his presupposition of harmony? How far can its efficiency go in exercising its collective power? Is its administration wise enough to know that its power is limited, and

strong enough to hold back its members who, in the exuberance of new-felt power, would go beyond that limit? Is it controlled by the Malthusian passion, stupidity and justification, or by statistics and reason? These are some of the problems in the process of evaluating institutions.

I know of no theory of value applicable to the complexity in this process of evaluating institutions except the theory of Reasonable Value, which the courts have been working over and over for several hundred years. Reasonable value is not a valuation of commodities or services. It is a valuation of the transactions and concerted practices themselves by means of which ownership of commodities and services is valued and distributed among individuals and concerns. The substance of this theory is that all of the factors must be taken into account and each be given what is called its "due weight".¹ This due weight, however, is not a process of logic or statistics, though logic and statistics may be useful as means of justification or crimination. Due weight is exactly what the words mean, namely, a due valuation of concerted yet opposing interests with regard to their relative importance in the national economy. This kind of valuation must differ with different investigators, different courts, different legislatures. It does not lead to deductive or conclusive results. All that the legal theory of reasonable value can do is to indicate possible lines of investigation. For it is historical and changeable. It is a problem, not a solution. It is a kind of valuation that changes with changes in concerted action. For it is a valuation that arises out of conflict of interests. It is not deductive because it is not founded on the economists' presuppositions of self-interest and harmony of interests. It is founded on conflict which is never logical.

Yet Reasonable Value does have a purpose, like all valuation. It is a valuation intended to bring harmony, for the time being, out of conflict of interests, by justifying the use of superior concerted power to compel harmony. It does not presuppose harmony. It creates harmony. Reasonable value is pragmatic, not logical; it is action, not truth; justification, not justice. The interests concerned are opposing property rights already determined by the preceding concerted action of history. The valuation of institutions is therefore historical and futuristic -- history, a process; futurity, its expected social consequences. It is an investigation of what concerted action does, has done, and can do. Such an investigation of concerted action is the investigation of historical and expected changes in the meaning of Reasonable Value.

These investigations, of course, are being made. The difficulty about them, however, is their invidiousness. They are investigations, not of commodities and utilities, but of purpose, efficiency and practices. No matter what their results may be, somebody will be hurt and somebody will be helped. Then crimination and justification take the place of reason. You cannot expect a political party, a legislature, a court, a business institution, a labor organization, to act on the cold investigations of science. What they want is justification.

1 Smythe vs. Ames, 169 U. S. 466 (1898).

Anyhow, the economic investigator is usually permitted to investigate no more than a small part of the whole institution. He is controlled by somebody higher up. He is not like the physical scientist who hurts only theologians. He hurts or helps politicians, or business, or labor, or agriculture. Hence the insistence that his investigations must be colorless. He is forbidden to investigate purposes or policies, and even some practices. These, indeed, give life and color, but they are invidious, secretive, and they deal with living persons. It is safer to investigate them post mortem. And the economist cannot even be sure that he is right, all by himself in his minute investigation of details. He may be mistaken. Yet he must take chances. To investigate an institution he must see it as a whole in the midst of many conflicting interests. An institution is a going concern. It is doing something to somebody, and it can be investigated and valued only as such.

Hence the new methodology is required. It is being worked out, more or less, here and there, by economic investigators. It is the methodology of giving due weight to conflicting interests. It is, perhaps, a method of concerted investigation by the conflicting interests themselves, aided by the economist. It is a method of neutralizing justification and crimination by statistics and reason. It is a slow method, a continuing and expensive method, a method that is always short of appropriations, and usually a disappointing and unacceptable method. It requires ingenuity, persistence, personality, toleration, and a thick skin. Such method is not suited to the older theories of supply, demand, marginal utility, cost of production, or harmony of interests. It includes them but transcends them.

It transcends them because it is historical and futuristic. An institution is historical because it is loaded with all the passions and traditions previously acquired by participants, to which we properly give the name Custom. It is futuristic because it carries the hopes and fears, the wishes and wants, the ambitions and suspicions, of all participants, in spite of which, however, they must cooperate in the present. Only when custom and futurity are included in the methodology can institutions be investigated and due weight be given to the conflicting interests that must be evaluated. We are only at the beginning of such investigations and of the methodology of carrying them on. This is because economics has not been a science of concerted action -- it has been a science of harmonious individualism.

Among the objects to be investigated and then given due weight in the process of evaluating institutions we may summarize a few. They are rather commonplace, after all, yet difficult. To what extent are the individuals competent for concerted action? To what extent are they willing to sacrifice self-interest for the good of the whole? To what extent do they get and keep competent leadership? What is their system of accounts, costs and forecasting? How far do they depend on themselves or on the State and courts? How far do they suppress and how far promote individuality among the membership? Do they over-reach or discriminate unfairly? How far do they modify prices, production and distribution? How far do their gains come from increased efficiency or from sheer increase in bargaining power? How far does their sense of responsibility go, and how much further can it be made to go? Wherein do they deserve support and

wherein restraint, in the interest of the nation as a whole? The answers to these questions are involved in the valuations of purpose, efficiency and control. Their investigation is the process of evaluating institutions as a factor in the world-wide economic changes of the future.

For agricultural economists the immediate occasion for evaluating institutions is in the program of the Farm Board designed to support agricultural cooperative marketing. I do not know to what extent the history of labor organizations may be of aid to farmers, but that history will at least indicate the important distinction between cooperative marketing and collective bargaining, a distinction which, it seems to me, has not been recognized or investigated in the case of farmers' organizations.

Prior to the decade of the 1850s, in both England and America, the labor organizations did not make the distinction. And the distinction was not generally appreciated until the period of Trade Agreements at the beginning of the Twentieth Century. The distinction is that between displacing the employer, merchant, or banker, by cooperation, and recognizing them as an organized unit by collective bargaining.

The distinction, indeed, reaches down to a fundamental philosophy regarding Capitalism. Prior to the 'fifties, especially during the 'thirties and 'forties, the abuses of the new capitalism were so flagrant that an opposing philosophy, Associationism, gained widespread acceptance. It took several forms. At one extreme was Anarchism, by which was meant voluntary cooperation. At the other extreme was Communism, by which was meant compulsory cooperation. The essential doctrine of all was the substitution of cooperation for competition. Labor organizations took up the idea piece-meal, and experimented with it down to near the end of the century. They tried cooperative marketing, to displace the merchant capitalist, by setting up their own wholesale warehouses. They tried cooperative production, to displace the employer capitalist, by organizing their own factories. They tried even cooperative banking to displace the financial capitalist. They tried consumers cooperation, to displace the retail merchant.

Some of these experiments survive, but in attenuated form, to the present day. Building and loan associations and credit unions are the cooperative banking of the 'fifties. The last grand attempt of labor organizations, however, was the Knights of Labor, which, with the Farmers' Grange, flourished during the 'seventies and 'eighties. But all of these labor cooperatives broke down for two reasons. The majority were unsuccessful, because it turned out that laborers were incompetent to elect the boss whom they must obey in the shop. The elections fell into the hands of politicians within the cooperative, and the issue became the factional one as to who should control the manager and make the rules which he must enforce on the members.

Neither could the cooperatives elect the business man who could master the intricacies of the markets. The successful business man cannot be elected by popular vote. He elects himself out of the struggles of competition.

And even if the cooperatives were successful, they still were unsuccessful. Success meant that their business expanded and they had to take on new workers. But those on the inside would not take in the new workers as cooperators - they took them in as hired hands. Hence the successful cooperatives became business corporations, and labor, as a class, remained where it had been.

But the trade union movement, which began in the decade of the 'fifties, abandoned all attempts, by cooperation, to displace the capitalists. They fell back to where they could do something by concerted action, namely, get more wages and shorter hours out of the capitalist system. They changed their philosophy from producing power to bargaining power. They left the employer in charge of the shop and endeavored only to fix the price of labor.

But even this was not collective bargaining. I call it labor dictatorship. I can best show what I mean by citing the labor organizations of San Francisco. For several years they controlled the building trades. They fixed their wages and hours, then took their schedules around to the employers individually and required them to sign individually on the dotted line. They called it "collective bargaining", but it was labor dictatorship.

That was a game which the employers eventually could play stronger than the trade unions. Suddenly the employers locked them out, and when the unions tried to break the employers' association they found that there were no independent employers with whom they could deal. The banks had affiliated with the employers, and an independent employer could not get credit. The merchants had affiliated, and an independent employer could not buy raw material. The employers called it "the American Plan", but it was, and now is, employer dictatorship.

Neither is collective bargaining. By collective bargaining both sides are organized. Neither employer nor employee acts individually. But the representatives of each draw up a joint agreement, fixing hours, wages and conditions. Then each individual contract between an individual employer and an individual worker is controlled by the joint agreement. This is what I mean by the Trade Agreement, which did not come to be understood until the beginning of the Twentieth Century. Collective Bargaining is trade agreements.

Any one familiar with this labor history can see its repetition in the farmers' cooperative movement. Already the commission-men on the produce exchanges are organizing on a nation-wide scale. They have obtained the support of the National Chamber of Commerce, representing hundreds of chambers of commerce throughout the country. They will obtain the support of the banks. The Chamber of Commerce, through its president, protests to President Hoover and to Chairman Legge of the Farm Board. The program of the Board will displace the middlemen altogether. The government is lending financial support to displace them.

As long as President Hoover and Chairman Legge are in charge, they will resist this opposition of the entire capitalist forces of the nation. But when they retire or get tired, the farmers will elect their own

managers, or politicians will see to it that less capable men are placed on the Farm Board, or Congress will cut the appropriations. When the farmers are left to conduct their own struggle, will they be able to elect competent managers? I doubt it.

The issue turns on the double meaning of "marketing". It means "production of wealth" and it means bargaining for its distribution.

The middleman is a producer. He manages the technical process of assembling products and physically distributing them. In economic parlance he creates "place and form utility". Somebody must perform this process. Can a cooperative perform it more efficiently than business men who have shown their capacity by surviving? Can these men be efficiently displaced?

The other meaning of marketing is bargaining and pricing. Collective bargaining would mean that the commission-men would be recognized as an organization with whom the organized farmers would, through their representatives, make trade agreements as to prices, deliveries, payments and other conditions. Instead of displacing them by cooperation, they would be dealt with by collective bargaining.

The one great advantage of the competitive system is that it shifts bankruptcy to individuals, whereas bankruptcy of a cooperative bankrupts an entire social class. If an individual business man fails, then his competitors absorb his customers, and business as a whole goes on. But if a cooperative fails then all of its members fail, and, worst of all, an entire social class loses confidence in each other and even in their government.

Collective bargaining is not a panacea. It has its difficulties, as well as cooperation. But it does this much: It lets the business man keep the chances of bankruptcy. There is one field of agricultural concerted action where it seems to be successful. I mean the fluid milk industry. The dairy farmers do not take over the marketing process by cooperative marketing. They only make trade agreements, as to prices and terms, with the middlemen who continue to do the marketing. They do not displace Capitalism, nor do they arbitrarily set their prices by agricultural dictatorship. They bargain collectively, and resort to arbitration, if necessary.

It may be that this example cannot be extended to other agricultural concerted action. I do not pretend to say. I only observe that it is the very process of evaluating institutions. Institutions cannot be evaluated until we are clear on our ultimate distinctions between cooperation, dictatorship and collective bargaining.

ANALYTICAL METHODS IN AGRICULTURAL ECONOMICS RESEARCH

By Dr. John D. Black, Harvard University.

Before taking up the discussion of analytical methods in agricultural economics, I shall have to say something about the nature of agricultural economics itself. It is customary to refer to agricultural economics as an applied science. Whether or not this is a safe designation depends upon ones understanding of the term applied. Alfred Marshall described an applied science as dealing with "narrower questions more in detail". So understood, the term applied includes all that we mean by agricultural economics. But there is another sense of the term applied, and in my judgment a more appropriate use, which makes it refer to the employment of the principles of a science in the performance of tasks. Thus the engineer, who is an applied scientist, applies the principles of mechanics in designing a bridge. And a government price analyst, the principles of economics and statistics in forecasting the price of some commodity -- other than cotton. Agricultural economics is much broader than can be included under the term applied in this second sense.

To understand what sort of a science agricultural economics is, we need to distinguish between two types of pure science, namely, general and special. The former comprises only universal principles, or those parts or phases of principles that can be stated in a broad, general way. The principle of diminishing returns or outputs stated in its generalized form is such a principle; or the so-called abstract theory of rent. Pure science in its specialized form coincides exactly with what Marshall calls applied science - principles stated more narrowly and in more detail. The principle of diminishing physical outputs stated as it works out when fertilizer is applied to varying types of soil under varying conditions, or when various feed rations are fed to dairy cattle at various stages of lactation - can be taken as an illustration of pure science in its special form; or the theory of rent as applied to actual farms, with buildings upon them and improved land. Obviously in practice, agricultural economics is both a pure science of the specialized type, and an applied science like engineering.

There is a tendency among some economists to say that there is only one sort of pure science, that of the general type, and that all the rest is applied. One can have no objection to this if the term applied is defined to mean what Marshall meant by it as well as its more common specification. No field of study is belittled in the least by being referred to as applied in the Marshallian sense, or as specialized pure science, the designation which I prefer. It seems clear that most of the progress in pure economic science in the present generation will be made in its specialized forms. The refining and testing out which the general principles of economics are receiving in the special fields of economics is exactly what is needed at the moment to make economics a vital forward-looking science. I confidently expect agricultural economists, working along these lines, to contribute more to the progress of economic science in the coming generation than any other group of workers in the subject.

One important obstacle to such a consummation, however, is the pressure under which so many agricultural economists are placed to do applied economics work of the second sort, because it appears to be more immediately useful. In the Land Grant Colleges, and I fear in the U. S. Bureau of Agricultural Economics, the demand is constantly that some group of persons shall be at once helped out of some pressing plight, or that some situation be relieved. Forced to respond to such demands, all that an economist can do is to go out and apply the existing body of principles as best he can. But the remedies proposed will be wrong much of the time, not so much because the principles are wrong, as that they have not been stated with sufficient clarity and explicitness so that they are properly applied by persons with the economic training now possessed by the rank and file of workers in our field. (I must confess, however, that the most frequent reason that the remedies so conceived are wrong is not so much the fault of the existing body of principles as the complete ignorance of them or misunderstanding of them, by those who are set to the task of proposing the remedies. I have no doubt that among those now earnestly seeking to explain price movements are not a few who are unable to define demand and supply, and state the laws relevant thereto, in the precise form needed for clear analysis of price problems.)

On other occasions I have pointed out what seems to be the way out of this dilemma. Few undertakings even of the clearly applied sort cannot be set up in such a way that they contribute something to the testing and refining of economic principles. Surely almost any project in price forecasting can be so set up, or any problem involving the amount of any factor of production to use, or the most advantageous proportion of enterprises. It is surely true of all the research that is basic to outlook analysis.

But if they are thus to be set up, those doing so must know the existing body of principles relating to the subject. They must in fact know it rather thoroughly. Otherwise the results obtained will not fit into the present pattern of our understanding. Because of a confused setup, the conclusions reached may have the effect of confusing the whole pattern. I could easily refer you to a study in prices, not of the narrowly applied type either, in which failure to comprehend the law of supply and demand leads the researchers to conclude that their data proves its falsity.

An even more important reason than the foregoing for knowing the existing body of principles is that only those who have that knowledge are sufficiently interested in their field, sufficiently awake to its scientific problems, and concerned with their solutions, to have the urge to set up their projects in remedial analysis in such a way as at the same time to test or refine some principle of economic science.

There is of course always some room, even in the Bureau of Agricultural Economics, for research projects of the specialized pure-science type. Surely after agricultural conditions become more stabilized again there will be more room in budgets for such projects. Also some of the applied work of the Bureau absolutely requires preliminary groundwork in pure science, and is so recognized. Fortunately this has been the case with much of the price analysis. But more important in the long run will

probably be the contributions that come as by-products of the remedial applied-science type of projects.

In the early days of the economic work of the Department of Agriculture, much stress in selecting personnel was placed on experience and knowledge of the commodity and the trade, and not very much on training. Those selected on that basis had for the most part what may be described as the "job" point of view, as distinguished from the scientific point of view. The natural tendency of most persons with this point of view is to be purely empiric in their approach to problems, to find out what ways and means seem to give the desired results, and then propose these as remedies. Pure science concerns itself with the why as well as the what and the how. The history of science is that progress slackens as soon as interest wanes in the why of things. Individual progress seems to be similarly affected. There is the adage, that "The man who knows how will always have a job; the man who knows why will be boss".

With the creation of the Bureau of Agricultural Economics, there was a great change in policy with respect to the economic work of the Department. Immediately the emphasis in choosing personnel was shifted more to training and somewhat away from experience. Workers in the Bureau were encouraged to improve their training, and training courses were set up in the Bureau itself. If there is any criticism now, it is that the idea of training is too narrowly conceived, being too much in terms of technology only.

One other important aspect of agricultural economics needs to be appreciated before we can set about considering methods more specifically. Economics is a co-ordinating science. It brings together in various relationships the facts and principles of the sciences relating to matter, man and society -- the natural sciences, engineering, the husbandries, psychology, sociology, and value and price. Such coordination is not possible without an understanding of value and price, and hence it remains for the economist, whose special concern is value and price, to make the final combinations. The economist of course has only a small part of the knowledge of the natural sciences, of engineering, the husbandries, psychology, and sociology that the specialists in the pure and applied phases of these sciences have, and he prosecutes no researches in these fields. Nevertheless if he is to coordinate successfully, he must have a general comprehension of these sciences, and oftentimes a fairly specialized knowledge of those phases of them that most closely relate to particular tasks of coordination. In many cases, he needs to call for the assistance and cooperation of workers in the basic natural and applied natural sciences, or in psychology or sociology.

The need for such comprehension of the basic sciences to be co-ordinated, and of cooperation with workers in these, is more urgent in the specialized phases of pure science than in the general phases of it. The refining of principles may lead one far into the intricacies of the basic natural sciences; as it does for example when the principle of diminishing returns is worked out in feeding and fertilizer combinations. Analysis of the strictly applied sort is even more dependent than the foregoing upon knowledge of basic sciences. Most actual problems of the sort which the applied sciences try to solve run more or less across the boundary lines

of the conventional disciplines. This often leads to attempts at their complete solution by workers in some one of these fields. The function of the economist is that of coordination, of seeing all the elements in a problem, then evaluating them and setting them in that relation to each other that will secure the largest total value of return. He may need the solutions of various parts of the problem by workers in the basic fields; but the final coordination must ordinarily be his. There are many natural science problems involved in rejuvenating the agriculture of the Old Cotton Belt; but the economist must have the final say as to what systems of farming are likely to make the area most prosperous.

In my further discussion, I shall not make any systematic attempt at a classification of research methods in agricultural economics. I shall instead select certain aspects of various methods concerning which more needs to be said than was said in the publication of the Social Science Research Council bearing the title "Research Method and Procedure in Agricultural Economics". The references will be less to statistical methods than their importance deserves - for the reason that in most discussions of method in economics, including that of the Research Council publication, less space is given to the non-statistical methods than they deserve.

First let us clarify our thoughts as to the relation between the genetic and the cross-section methods of analyzing situations. The genetic method of analyzing situations consists of tracing their evolution, of showing how they got to be the way they are now; the cross-section method consists of determining what conditions or circumstances are associated with it now, and presumably are responsible for keeping it the way it is. Many researchers incline strongly to one or the other of these methods of analysis, giving scant recognition to the possibilities of the other. There are those who are entirely content with an explanation when they have shown how a condition has evolved; and others when they have discovered what is immediately associated with it. These two points of view were well illustrated in the conflict of opinion that developed between an anthropologist and a psychologist as to the best way of explaining certain phases of human conduct, before the Committee on Scientific Method of the Social Science Research Council a few summers ago. The difference between these points of view is perhaps more simply illustrated by the problem of explaining types of farming. The pure genetic method would consist of tracing the changes in crops grown, live-stock kept, etc., over a considerable period in the past and leading up to the present; the cross-section method would endeavor to explain existing systems of farming in terms of climate, soil, markets, competition, prices, labor supply, farm practices, etc.

Most of us would say that the two methods supplement each other. We would concede that "the roots of the present" lie somewhere in the past and that the significance of present associations is greatly increased if we know whether they are new or have long continued, or can trace their origin and history. But we would also say that an explanation that is genetic only is far from adequate, that it is not sufficiently complete on the one hand, nor definite and quantitative enough on the other. To take again as example the problem of explaining system of farming in an area: we would not be satisfied to project future developments by the pure

method of extrapolation, of projecting trend lines and associations out of the past. On the other hand, we would not be content to assume that because certain factors are associated with the present system of farming, they will continue to be associated with it in the future. What we want to know is a combination of these two. The ideal is attained when cross-sections is taken at many points in the past, each of these is carefully analyzed in terms of its contemporary associations, and then the various cross-sections and their explanations are placed in time sequence to discover what elements have continued to exert an influence throughout, which have been temporary in their effects, and which have waxed and which have waned. If we were interested in explaining the land values of an area, for example, we ought to have not only a cross-section of the present showing all the circumstances that may be involved in establishing the present level of land values and variations in the same throughout the area, but a similar analysis of other cross-sections at five- or ten-year intervals, back far enough to establish the direction of the important trends. Such an analysis will show whether distance from market has had an increasing or a decreasing influence on land values; and the same for concentration of urban population and the other important factors.

Usually, however, the data are not available for very much of a cross-section analysis at various cut-offs in the past. Instead we have to be content with a certain amount of occasional, unsystematic, largely non-quantitative, descriptive information, with possibly one or two significant series of data running back over the period. We are therefore not able to establish any clear set of associations for cross-sections in the past, or observe the trends in the same; but must content ourselves largely with projecting the trend for the dependent variable alone. If we do try to trace a few associations with the dependent variable up to the present, in most cases these will be a matter of hypothesis - because not sufficiently substantiated by other data and analysis. We do not accept the mere coincidence of two series as proof of causal relation in cases where we have data to test out other possible relations; and why should we accept it as proof where we do not have such other data? There is a saying that history has to be written over again each generation. This is largely because the historians of each generation discover a new lot of coincidences and promptly set them up as established new causal relations. Could a rather full cross-section picture be obtained for a succession of cut-offs, we could look at all the coincidences at once.

The genetic approach just described is of course historical. But not all historical analysis is genetic in its purpose, that is, aimed merely at showing how an existing situation came to be. Historical analysis may be directed also at setting up principles. Genetic analysis may in the end contribute to the establishment of principles; but this is never its immediate object. Historical analysis develops principles by the procedure of observing the co-variation of the different elements in a problem in time sequence. We shall establish our body of principles relating to price by determining the factors that are associated with differences in price from place to place; and also those that are associated with changes in price from time to time. The latter is historical analysis. The same historical approach may be used in establishing the principles relating to the combination of enterprises in farming - we may observe the changes that have

taken place in the past and the other changes that have been associated with them.

Before going further, we must make sure that the distinction between qualitative and quantitative analysis is clearly discerned. Qualitative analysis answers that question what; quantitative analysis the question how much. The correct use of these terms in social science is parallel with their use in chemistry - qualitative chemistry contents itself with saying what elements are present in any compound; quantitative chemistry goes further and attempts to tell how much of each element is present. It is necessary to be this specific because I have found in my graduate teaching that my advanced students come to me with loose notions to the effect that qualitative analysis deals with "qualities" of things - such as the taste of butter, the managerial ability of farmers, type of soil, country of birth, form of land tenure - that cannot be reduced to numerical measurements. It is true that analysis relating to such properties is usually only qualitative; it is not possible to say how much of any of these properties is present in the independent variables and hence not possible to measure their effect on the dependent variable. But the great bulk of qualitative analysis concerns itself with factors in situations or relationships that are entirely capable of being measured quantitatively. That they are not so measured is due to the indisposition of the analyst - not to the nature of the factors involved. The principle of diminishing physical outputs is purely qualitative in its usual form of statement; but it can easily be reduced to quantitative form for any particular combination of the elements of production. The theory of land values may be stated qualitatively in terms of what factors determine the value of land; but it is possible for any particular area to state at what rate each of the elements affects land values.

Now for very good reasons, in practice a large part of genetic and other historical analysis is only qualitative; and in consequence many people have formed this habit of thinking of all such analysis as qualitative. On the contrary, an increasing amount of historical analysis is statistical. All of our recent study of movements of prices of particular products, of business cycle movements, of the behavior of prices, is largely quantitative in its nature. Statistics has developed the whole mechanism of time sequence analysis with a view to its use in genetic and other historical analysis. Persons with statistical training are doing an increasing part of the real historical research of the day.

But after the statistically minded historians have done their part, have analyzed all the existing data of the past, have gone out and collected vast amounts of additional statistical data from books of old business firms, from old town records, old newspapers, old diaries, etc., there will still be many important changes in the past, many important events or developments, for which we will have no quantitative records. We shall have to be content in such cases with observing coincidences and examining them carefully as to their plausibility and for supporting evidence. The effort in such cases should be directed at getting as complete cross-sections as possible in non-quantitative terms, and tracing the changes in the elements comprising the cross-sections.

The method of analyzing non-quantitative information concerning changes in the past, is not discussed at all satisfactorily in any literature commonly available to economists, if available anywhere. When the historians discuss methods, they talk mostly of testing the authenticity of the source material - which is surely a most important matter. What they say about analyzing the information after its validity has been substantiated is usually in vague general terms. The name the historians give to the operation is "historical synthesis". (See Dr. L. C. Gray's discussion of this on pages 348-350 of "Research Method and Procedure in Agricultural Economics".) The mental processes involved are of course those which are commonly analyzed under the head of logic; and very few economists know much about logic. There is great need that someone will work out for us a body of material comparable to that now in our textbooks of statistics, but designed to help us think straight, first, concerning relationships between changes in time sequence reported in non-quantitative terms, and second, concerning relationships between differences between areas.

Historical analysis has an important advantage in its favor that in part offsets the prevailing paucity of data. If a long enough period is included under observation, many of the elements in any one cross-section that appear important as far as that one cross-section is concerned, have time to prove their unimportance. Time itself becomes a powerful agent in selecting the universals. Those elements which have stood forth over a long period can be accepted as significant with much more assurance than those seeming to weigh heavily in the present only.

It should also be pointed out that much data supposedly only qualitative is roughly quantitative. The measures of amount or importance of the variable may be in very approximate terms, gross fractions and percentages, rough estimates - but still quantitative. When such is the case, a certain amount of quantitative analysis is possible.

It should be apparent also that where data are available for a long period, it is not necessary that they be especially accurate. An error of five percent over a 50-year period is several times less serious than an equal error over a 5-year period.

Even where considerable quantitative data are available as a basis for historical analysis, they are never complete. At the best in time series analysis of prices, there are important residuals in many years for which an explanation must be sought in conditions not recorded in numerical measure - such conditions as changes in styles, the psychology of the market, pooling programs, poor keeping quality of the crop. The analysis of such factors must make use of the best technique of the historian in collecting and analyzing the facts. The usual research problem is one in which a mixture of quantitative and non-quantitative information is available, and therefore a combination of the two techniques is required in the analysis.

Agricultural economists always have made much use of the geographic approach to the study of their problems. Most of the analysis relating to it has been either purely qualitative or informally quantitative in character. To be sure, there have been data as to the number of acres of the

different crops and the like, and these have been duly represented as quantities on maps; but the analysis following has been mostly in terms simply of whether certain factors coincide or not in area; or in some cases whether greater or lesser amounts of them, very roughly expressed, coincide or not. Only recently have economic statisticians been applying the methods of statistics to such analysis. (The climatologists of course have for some time been using statistical methods for certain types of analysis, such as the relation between rainfall and yields.) There is now a growing realization that close analysis of differences in the dependent variable from place to place, and accounting for these differences, may assist greatly in developing principles. Also in explaining a given situation. For example, I may learn as much that is useful about the reason for the present level of land values in New England by observing and trying to account for differences within the area as by observing and trying to account for the change that has occurred in the general level. Along with a growing appreciation of the importance of such analysis must come a development of suitable technique in analysis, as distinguished from technique in mere map making.

This technique must be both qualitative and quantitative. Historical analysis has to deal with many incidents of minor importance and some of major importance that are special to given years or short periods of time. Isolating the effect of these is one of its major problems. Geographic analysis similarly has conditions both of minor and of major importance which are special to the different parts of the area, whose effects need similarly to be isolated. It would appear that these special factors furnish a more serious problem for geographical analysis than for historical analysis - they probably happen with more irregularity and with less apparent design. Under the circumstances, qualitative analysis of geographical factors will always have to be strongly mixed with the quantitative.

Many problems of analysis which cannot properly be called either historical or geographical and which lend themselves only to qualitative attack, are constantly arising. A common type of such a problem is the description of the marketing process for a commodity - of the agencies handling it and their organization and business practices. Another type is the working up of a set of grades and classes for a product. A description of a system of farming is another type. Dr. Nourse has referred to those as "qualitative description" in his section of "Research Method and Procedure in Agricultural Economics" (pages 324-328). The logic of such analysis relates to the process of classification more than to any other intellectual process. This involves such steps as determining the attributes of the individual cases, grouping these attributes, finding the relations between them, determining coordination and subordination among them, distinguishing between bases of classification, and finally the setting up of a set of definitions.

When the differences between individual items can be measured, then they can be arranged in order from most to least for any attribute, and hence reduced to frequency tables and averaged. "Qualitative description", as Dr. Nourse uses the term, under such circumstances takes on quantitative form in its final stages.

One hears much these days in certain quarters of the importance of the institutional approach to economic problems. As far back as 1915 to 1918, when I was a graduate student at Wisconsin, the institutions figured largely in our day-to-day discussions. None of us ever learned what an institution was. Sometimes the term was used to refer to some long-established type of social group, such as the family; sometimes to refer to some long-established social arrangement, such as property or contract, or personal freedom. Today we find economists also referring to a board of trade or a produce market as an institution. Probably the essential idea of the concept institution inheres in its age - a sufficient period of time must have elapsed so that a certain social force has developed which imposes itself on the currently arising situation and modifies the final results to some extent. A large number of types of social arrangements have thus acquired at least a degree of social force -- all our established types of business institutions, public institutions, and social institutions generally, and all our habitual or customary ways of doing things and thinking about things. In fact, we can say that broadly the institutional analysis involves the whole existing structure and functioning of society, insofar as these at any point have attached to them any social force developed out of past performances by the custom-making process, which predisposes any part of society to react in a certain way to the new situations that arise.

A complete institutional analysis would need to be as broad as the foregoing. In practice, it is usually limited to a few of our major social arrangements that have acquired a considerable measure of social force, such as trusts, corporations, labor unions, forms of land tenure, etc.

Now that we have determined the nature of the institutional approach to social problems, what about the analysis that accompanies it? I fear that I am compelled to state that thus far institutional analysis has reached the point only of cross-section description and historical narration in qualitative terms. The institutional writers content themselves with talking about the institutions, telling us what they are like and along what lines they have evolved. Hence the technique involved has already all been presented in the foregoing sections. The time has about come when the institutional researchers will need to do something more than talk about their institutions, when they will need to weigh them and measure their attributes, and then show the amount of social force which they have exerted in the past, and are exerting in the present. In other words, they must discover ways of putting them into problems of analysis as the independent valuables which we commonly recognize them to be.

Lastly, let us briefly consider the case method and its place in the scheme of research. Agricultural economists have long used it. They have made literally thousands of case studies of farming areas. They have made thousands of case studies of individual farms, individual marketing units and the like. To be sure, they have not always realized that they were making case studies. Often they have made the mistake of trying to work out statistical tables and averages for small groups of farms or other units that should have been treated as units in a case analysis. This leads to a statement of the essential difference between the case and the statistical method. In its typical form, the case method examines a few selected units completely and thoroughly and arrives at its conclusions by

the method of noting likeness and difference, whereas the latter examines a larger number of units, enough to make a sample, abstracts a few of their attributes, measures these, and then summarizes these measurements into frequency distributions, averages, correlations and the like. In case analysis, a careful examination of all the related attributes and attendant circumstances serves to protect the validity of the generalizations. In statistical analysis, the method of procuring the sample and the size of it are supposed to protect the generalizations. In particular, it is assumed that variations due to the remaining attributes will offset each other's effects, and hence can be ignored for the most part.

The foregoing should make clear the mistake involved in applying statistical methods to units in a case study; yet that is what has frequently been done in the past in the so-called cost-route studies of farming in the United States. Even now, there is need that someone will give a good example of thoroughgoing case methodology applied to the analysis of data collected from a small number of farms in a route. What type of farms will be selected: several which are closely alike so that they will serve as checks on each other? Or several which differ from each other in essential respects so as to observe the effects of these differences? Or a combination of the two foregoing, several sets of farms differing from each other in essential particulars? These are only a few of the possible bases of selection. Shall all aspects of the business of these farms be studied, or only a group of related problems, with such general background as is needed? It is generally assumed that case studies must include the whole organism; but as a matter of fact they may be limited to certain parts or phases as long as these are explored fully. Exactly what data shall be collected and how shall they be analyzed so as to yield safe generalizations?

So much has been written and said about the case method by workers in the general field of the social sciences that it would seem that one ought to be able to turn to them for insight in the processes involved in case analysis. But unfortunately one obtains little real help from such sources. We are told in such literature that there are two possible procedures, to seek for differences, or to seek for likenesses. Logically, of course, this is only one procedure. In effect, then, what we are told to do is to observe how and to what extent the second unit we examine is like the first one, and the third like the first two, and so on until we have examined them all. But surely much more than this is involved. Surely we must note what differences and likenesses tend to be associated with each other and try to get back of these associations to discover plausible reasons for them. We must discover the relations between all the parts of each unit. We must develop methods of synthesizing the various properties of the different units and noting the effect on the total product of various combinations of the properties. When we get through, we ought to know just how effective relatively each unit is, and the reasons that it is this effective.

Genetic analysis is commonly considered an important part of case analysis; and properly so. Tracing the evolution of each unit up to its present condition should throw much light on it and help us to understand why it is set up as it is and functions as it does. Out of the differences

in the genesis of the different case units should come generalizations as significant as those based on inter-relations within the existing unit.

When a unit in a case study is large and complex of itself - as when line elevator systems are the units in a case study, or chains of farms, or a type of farming area - there is room for much statistical analysis within the case itself. But the method is still the case method if the several units in the study are set alongside each other case fashion instead of being combined into frequencies and averages, etc.

The number of units in the study is not the determining factor whether the method employed is case or statistical - it is the analytical procedure that is determinative. Ordinarily, however, the case method is used only with a small number of units - no more than the investigator can keep in his mind as individuals at one time; and the statistical method requires enough units to make some kind of a sample.

The case method, properly understood and properly used, has an important place in economic research. We are going to find increasingly that certain types of units can only be studied to advantage by the case method, that not enough sufficiently homogeneous units are available to make statistical analysis profitable. After statistical methods have been applied for a while with units which are numerous enough for it, we will find ourselves turning to case methods in order to obtain a closer look at our problem and single out new phases of it to attack by statistical methods again. We therefore need to be giving more attention than at present to the technique of case analysis. At present most of what we know about it relates to methods of collecting the information.

Conclusion

It should be clear from the foregoing discussion of specific methods and approaches that research workers in the Bureau of Agricultural Economics and in the land-grant colleges have plenty of opportunity to contribute their share to the development of principles and of methods of research even while centering much of their efforts apparently on particular situations. They are doing this in an important way at present.

There is need for a warning, however. The pressure of work under which many of us labor unavoidably gives us a job point of view. I find some of the best thinkers in the Bureau occasionally expressing impatience of "economic theorizing". Methinks this is evidence of degeneracy. At least I feel it so in my own case. It may seem strange to you that persons devoting nearly their whole time to teaching and research in pure sciences find themselves slipping also. But such is nevertheless the case with many of us. The reason is that our actual teaching and research tends to be restricted to special subjects, and we lose our sense of the whole and the inter-relation of its parts. Therefore, sabbatical years are set aside for us.

If we need them, how much more do people need them who spend a large part of their time and energy in the day-to-day grind of preparing reports on the current situation, or looking into problems that need

adjustment. Verily the most unfortunate of such are in the position of the practicing doctor who has not had time to read an article in a medical journal in years.

The Bureau of Agricultural Economics has given evidence of remarkable vigor of growth since its establishment. It has manifested a scientific spirit that has thus far risen above all its repressions. The continuing success of its system of graduate instruction furnishes evidence that this growth will continue.

FACT AND INTERPRETATION IN ECONOMICS

By Dr. F. H. Knight, University of Chicago.

My task on this occasion is one to be approached with misgivings, and I do approach it with doubts. I do not see clearly and surely in the field of economic methodology, and the airing of doubts, or viewing with alarm is likely to be thought an ungracious performance. Nobody loves a bear! But after all doubts have their place. We do not get where we want to be by driving with enthusiasm and power and speed in the wrong direction. And I do feel strongly that some present trends in economic activity carry more than a threat of wasted energy. If the effort to solve a problem is to be fruitful it must be put forth in the light of a correct conception of the nature of the problem itself, and there can be no real gain from conceiving a problem more simply than it really is, and thus make the solution appear easier.

My reference is of course to the current enthusiasm for making the study of economics "scientific," meaning factual, concrete and quantitative, or specifically, statistical. I have to raise questions and suggest doubts as to whether the proper content of this study, or "science" can really be facts, whether it can really be a "science" if we use the term in the sense it carries in speaking of the natural sciences. As the subject announced is intended to suggest, I must argue that Economics deals rather, primarily, with meanings with what facts mean rather than facts themselves. Consequently, while of course we have to consider facts and be careful to get them "right" we have to approach them, and look at their rightness and wrongness in very different terms from those proper to the natural scientist; for the economist or other social scientist, in this view, facts are preliminary, not the real subject matter of the study. The main theme of these remarks will then be the contrast in character and method between the natural sciences and those which deal with man in society, with particular reference, of course, to economics.

At the outset, however, I want further to say that I understand the feelings of those who want to make economics an objective and quantitative science, and sympathize with them deeply. The "backwardness" of the studies dealing with man, in comparison with those dealing with nature, is superficially an obtrusive fact, and one which seems superficially to point its own moral. In the face of the contrast between the solid achievements of the natural sciences in the past few centuries, and the relative lack of advance in the understanding or control of social relations since the Ancient Greeks, it is natural to conclude that the way to reform the social sciences would be to imitate those which appear so much more successful in their task. And in particular, it is natural to hit upon the theory that the social sciences have "remained" in the "speculative" stage, while the natural sciences have taken to careful detailed observation, measurement and experimentation. In the face of this situation, to repeat the thought in more vernacular terms, it is most natural to develop a certain impatience, to insist on getting out of the stage of speculating and arguing what to do, and do something, and to put content into this by making it

mean to get the facts, bring them into relation with each other and see how they may be used for prediction and control, as the physical sciences have been so successful in doing.

However, a little examination will show that the case is not so simple as that. To begin with, we have long had natural sciences of man and they tell us nothing about social events. The physics, chemistry, biology, physiology and pathology of the human organism are extensively studied and well developed and beyond a few broad and obvious statements, mostly negative they do not reveal anything about the course of history, or make possible the prediction and control of social movements. We know that human beings will always eat, and that if they live in certain climatic zones they will have some protection from the elements. Perhaps we may add speech and recreation as biological traits. But such general information is of no concrete use to the economist, for example. To be useful to him it must go so much farther, into so much greater detail, as to what people will eat, wear, etc., and how much, and how, that the problems become different in kind as well as degree. As soon as we try to make general statements in this field, we find that any general import they have runs in terms of something quite other than the facts observed by the senses. The uniformity, as suggested already, is in the meanings, not in the concrete content of behavior. Even in the matter of food, it is men's knowledge or beliefs about what is desirable or "fit" to eat rather than that actual physical qualities of materials which are decisive.

The best illustration in principle is in the field of communication. The sounds and characters are physical facts, but there is practically no discoverable relation between these and what they are used to convey. If we know anything for sure, we can say we know there is no connection between language differences and either physical differences in the peoples or the content of thought or emotion they wish to communicate. It appears that any person could equally well learn any language and, that with slight reservations, not important in this connection, any language can equally well express any content that is expressible.

The function of the natural sciences is to describe the properties and "behavior" of things as they appear to our senses, that is, physical things and materials in space, and behavior which reduces to rearrangement of matter in space. The essence of it is the descriptive point of view. It tells what happens, not why anything happens. From the "pure" science point of view itself (separated from practical significance) it enables us to understand the complex manifold of events in the outer world by reducing them to a manageable number of elementary general principles, especially and perhaps at last entirely, those of mechanics. It does this by finding "uniformities" or "repetitions" in events, by showing that under similar conditions similar consequences follow. Thus Newton showed that the movements of the heavenly bodies exemplify the same phenomenon of "falling" that is familiar for objects near the earth's surface; and Darwin showed that the production of the infinite variety of plant and animal forms might be viewed as a working out of the same principle as the production of new varieties through selective breeding by the gardener or fancier.

Back of this function of science of enabling us to understand things, of explaining and so satisfying our intellectual cravings, is, as we all

know, the practical function or functions, of making possible prediction and control. The fundamental point here, which seems to be overlooked in proposing to make the social sciences "scientific" is that the natural sciences themselves are based on the assumption of a sharp antithesis between man and nature. Man is the controller, nature the to-be-controlled. In fact, quite aside from this practical relationship between user and used, workman and tool, the same insuperable opposition really holds in the mere logical relationship between knower and known, or understander and thing. or matter understood. But it is clearest in the practical view. All our notions of prediction and control, by man over nature, through science are bound up in a conception of nature as passive, over against ourselves as possessed of mind, will and initiative. It is never trying to control man. More specifically, we view nature as an aggregate of things and materials in space, purposeless and inert in themselves, completely amenable to "control" from without in the particular sense of being movable from one place to another, which movement may liberate potential energy stored up in them, or modify the process of storing up or releasing such energy in some way.

When we examine the notion of prediction we find that it reduces either to the fact of "inertia," the property of things by which they stay where they are or keep on moving as they are moving at any time, unless "acted upon" in the sense of having motion (or some new motion) imparted to them from without, or to the release of potential energy. The notion of control is always relative to movement because the only way in which human beings can act upon the external world or produce any change in it is through our voluntary muscles, which can directly produce only the change of moving some bit of matter from one point in space to another. All changes which man produces and which constitute his "control" over nature are the results in nature of such movements of matter if they go beyond the immediate fact of motion itself. Most of our knowledge of nature, the content of the sciences, which gives variety and significance to our control activities, consists of facts regarding the processes (always the same under the same conditions) according to which energy is stored up in or released from natural materials in connection with their spatial relationships. The amount of energy communicated to natural objects by our muscles directly is generally negligible, though such a movement as striking a match may start energy changes which will explode a magazine or burn up a city.

The point here is merely that science itself depends on the assumption that just as things do not move or change their state of motion of themselves, they do not change their behavior in storing up or releasing energy of themselves, but do change as to these processes in uniform ways in response to outside acts of the form of moving them about in space in relation to each other. These uniformities are physical. A natural process, for instance, may be set off by a sound. It is said that avalanches have been started by sound waves. But in nature, the same sound will always produce the same effect. Sounds, and other causes, act as what they physically are, and not as symbols or bearers of meaning. Let us consider the contrast between this situation and that presented by the problem of applying scientific method in the field of the study of man.

In the first place, we must again note, human beings are undoubtedly natural objects, things in space, and as such they seem to be subject to

all the laws and principles which science finds to hold for other objects under the same conditions. The same principles of physics and chemistry and physiology apply in the human body as elsewhere, as far as the most careful measurement reveals. But in addition some other principles seem to apply which do not hold good elsewhere. Men are more than mechanical objects which release energy in uniform ways in response to external movements of matter. They initiate changes, out of all discoverable uniformity of relation to external changes of any kind; and when they do respond to external changes, the nature of the response has relatively little uniform relation to the physical nature of the stimulus but is chiefly a matter of what we call the meaning of the stimulus-event which puts the whole occurrence, as the philosophers say, in a different universe of discourse. These meanings and the responses to them depend on the history, which is a thing made up of meanings, of social groups and the particular life-history of the individual in the group; and they are very largely free from "dependence" on anything which research has yet disclosed. As far as can be judged in the present state of knowledge (in the speaker's opinion) the problem of understanding and explaining these phenomena must be approached in a quite different way from that of understanding and explaining physical nature. (In the scientific sense I mean; ultimately, philosophically, the problem of explaining nature is itself likely very different from that of science, for as already noted science does not pretend to give any answer to any question of why things are as they are.)

The root of the difficulty in regard to explaining and controlling human beings is the fact that the explainers and controllers are likewise human beings. It is impossible to regard human beings as of one kind when understanding and exercising control and of another and totally different kind when being understood and controlled and yet the two roles call for different characteristics. I shall return to this point presently. For the moment I wish to go a little more into detail about the "more," in the statement that man is more than an object in space behaving in relation to other objects in accordance with universal mechanical principles.

It is possible to look at a human being in several strongly contrasting ways, and describe him in different sets of terms. We may look at him, for example, in psychological terms, and "explain" his acts by relating them to mental states. Many changes can be wrung on this theme. The philosopher Hegel gave a logical or dialectical interpretation of history, and the British psychologists of the early nineteenth century explained human nature in terms of association of ideas.

Another possible approach is in terms of "institutions," a term which is being much used in economics these days, and very loosely used, and largely misused. An institution in the proper sense is a phenomenon of the nature of the language. It is neither a mechanical response to a physical stimulus nor a deliberately contrived procedure for achieving an end. Language is of course a tool, it is seen to be one after it has developed, but no one ever contrived it (in so far as it is a pure type of institution). It is believed by students of the subject that language actually developed primarily as a vehicle of emotional expression and acquired its more utilitarian functions secondarily. In any case, the methodological point is that the student of language treats it as an entity on its own account, indeed without very express reference to human beings

or their interests and acts. It seems to have its own laws of relationship and of change, much like an organic species. It is a figure of speech, but a descriptive one, to call the human group the soil in which a language, or other institution, grows. Just as the plant one gets depends on the seed sown and not to any great extent on the soil, so it seems that institutions grow and change without much reference to the human beings who carry them on -- though sensitive to contact with other related institutions with which they may hybridize, again much like plants.

There is much justification for an "institutional" approach to what we call economic phenomena. If we look at the facts of wealth and the processes of its production, distribution and consumption, and ask "why" these things are as they are, it is a very defensible answer to hold that they are customs which have grown up, much in the way in which a language grows, and to be "explained" only by giving the details of the history of that growth. Such an interpretation should, it seems clear to me, be kept very distinct from the "statistical" approach to the same problem. Economic statistics stand as a method at the opposite pole from institutional history. There is little or no distinctly human content of any kind in them. They relate almost entirely to commodities as such, and to external means of economic life rather than that life itself.

It is to be noted that the traditional or orthodox economic thought, in the British utilitarian line, is very different from both of these; in fact institutionalism and business statistics represent reactions in opposite directions from the utility-and-cost, supply-and-demand economics. The conception of human nature involved in the latter is interesting and needs to be clearly understood. Man is not looked on as a physical behavior mechanism, or a psychological being, or as the bearer of institutions, but as a being who has wants and limited means for satisfying them, and who is confronted with the problem of making the means go as far as possible. The means and ends of action are data, the procedure itself problematical. This standpoint will be clearer if it is contrasted, on the one hand, with a mechanical view of human nature, in which the response is completely determined by the conditions and hence is not in any sense problematic, and, on the other hand, with a view (or with a type of situation) in which action is conceived in terms of means and end but the end is also conceived of as problematical. As I myself see the matter the view of "unsophisticated common-sense" is in the main that of the classical economics. We assume that people in general know what they want, and are confronted with the problem of getting it, in the maximum degree, with the limited means at hand, which problem they "solve" more or less completely, through intelligence or luck. The problem itself, the ends to be realized and the means and conditions are given in the person and his situation, but his activity in "solving" it is peculiar in that it involves effort and in general a greater or smaller margin of error, these being absent from mechanical reactions.

When we look critically at human behavior, it seems to me that we are forced to recognize that the ends of action are problematic in about as great a degree as the means. Life seems to be an exploration as much as it is a quest in which we know what we are trying to find. This conception might be designated by speaking of the ethical man, in contrast

with the economic man and the mechanical or behavioristic man, a variation of which would be the institutional man.

The difficulty is that all these views, and still others which I cannot here even list, have some degree of validity, and yet it is most difficult to make them seem consistent with each other. The philosopher Kant gave effective statement to a part of the problem, the conflict between the mechanical and ethical view of human nature, in his famous statement that man is at once subject to universal causality and a self-legislating member of a kingdom of ends. As I see the "facts" - which are facts in the sense that everyone treats them as such when he is not expressly trying to prove some theory - the situation is much more complicated, and hence much "worse" from the standpoint of our intellectual cravings and practical needs for simplicity. We seem to have to reconcile ourselves to the fact that man is at once not merely ~~two~~ but a great many different kinds of being, kinds which seem logically contradictory. He is different kinds under different circumstances, or capriciously or accidentally, and he is even several kinds in the same situation. He is a cause-and-effect mechanism and a bearer of culture or "soil" in which institutions grow according to their own laws of growth, a being of irrational judgments and a being who deliberates and decides intelligently (more or less!) and this both regarding procedures for reaching ends which he accepts unconsciously and also about ends to be chosen and pursued. For anything like completeness we should have to add still other items to the list, such as that he is commonly and in all sorts of degrees a dreamer and mystic and even an intrinsically "contrary" being and often takes a perverse delight in being thwarted and punished and in having grievances against the world and all and sundry in it.

It is indeed a formidable if not forbidding task to theorize about such a creature or formulate generalizations in terms of which his actions can be predicted and controlled. But it is hardly in conformity with the scientific attitude to insist on false simplification or refuse to face the facts because they present difficulties. The contrast between the problem of prediction and control in the case of a mechanism and in the case of human beings may be seen in a number of kinds of simple illustrative cases. In the first place, the entire theory of science depends, as noted above, on the repetitiveness of events and uniformity of relationships; the same effects follow the same causes. But in the mere external facts of the case this is not true of human beings. Physically, chemically and physiologically they are alike, enough to infer from one case to another, within limits, though it must be remarked that even in this field the science of medicine is seriously embarrassed by unaccountable differences in the reaction of different cases to the same treatment. Moreover, the doctor, if candid and shrewd, relies perhaps as much on psychological treatment wisely varied to fit the case as he does on drugs and physical therapeutic agents. On the plane of social behavior, however, even this minimum of uniformity seems conspicuously absent. Experiment with one human being simply does not tell how another will respond to the same experiment, as nearly identical as it is possible to make the repetition.

And worse, it is in the very nature of the creatures that the same one will not ordinarily respond in at all the same way if an experiment is repeated. Let anyone try the simplest experiment, such as telling another

a story or sticking him with a pin or offering him a present of a five dollar bill, and then repeat the "stimulus." It is, as just stated, the very nature of a human being not to be at all the same person with reference to a repeated situation as to its first occurrence. A gun or a trap which has been discharged or sprung is, when reloaded or reset, the same as before, but you cannot restore a person to the original condition, even to the degree within which it is possible to find another like him. People are different from mechanical objects in that they have a history. In part this difficulty may be avoided by taking them in groups, but groups also are always unlike and each group has a history. None of us is like his forefathers, even in the tenuous sense in which he is like his contemporaries. Our "situations" are very different, and our responses are different even where the situations appear similar.

This does not mean that the case is hopeless, that there is no place for intelligence in human relationships, or even that it is impossible to effect improvement through diligent observation and study. Our everyday experience proves the contrary. With all our bewilderment, we do have a fair knowledge of what to expect of our fellow-beings in ordinary situations and of how to treat them to secure cooperation and orderly living. It is a question of method. We do not acquire our common-sense knowledge of how to get along with our fellows in the same way as our common-sense knowledge of how to respond to and use natural objects, and it is reasonable to suppose that in the one case as in the other improvement will be secured by refinement along the general line of common-sense procedure. The essential fact in understanding our fellow human beings is primarily that we communicate with them. Thus in a sense we get inside of them instead of merely observing them from without. Of course our communication is based upon external observation, but the essential difference remains.

It is impossible to elaborate upon this difference here, and it should not be necessary. The heart of it is the contrast between a more direct instinctive but unformulated knowledge, based on familiarity on the one hand, and, on the other, reduction to rule in terms of physical units. A good illustration is the learning of a language. We can and do, without great difficulty, learn the meanings of sounds and characters and recognize them with fair accuracy and with little effort. But to base such knowledge on physically measured specifications as to the precise wave-forms or shapes would be quite out of the question practically, though a certain amount of such study may be interesting afterwards. The principle holds throughout the field of human phenomena and relationships. We describe people and works of art and literature and other products with a fair degree of intelligibility, and recognize them by their traits, though we could not make a beginning at putting this knowledge in accurate, scientific, physical terms. (Of course the artist who wishes to simulate effects in a physical medium does have to know in a sense how the lines and colors go, but his knowledge is also an immediate feel of how to do the thing and nearly as far remote from the ideal of mechanical "directions" as is the interpretative recognition of the layman.

My concrete suggestion is that if economics and the social sciences want to make more rapid progress they must give up the visionary ideal of building a society from blueprints and dimensions as we build a house and quit trying to imitate engineering and the sciences upon which it is based

and turn rather to the study of their own data and the processes by which we do come to have some intelligence in relation to these data on the level where progress has already been achieved. That is, we should learn from "art" in the broad sense, and from the way in which the arts are learned and taught rather than from physical science and engineering technique.

It is to be admitted that in an important sense this is less satisfying. Our minds to crave the definite rule, the fool-proof formula. But it is a question of facing facts, and the actual character of the problem. It will never be as simple and definite a matter to improve the grammar or the morals of a social group as it is to build a bridge or compound a chemical. But we shall not make the task easier by insisting on applying methods which would admittedly be more satisfactory if they could be applied but which simply will not work because it is not that kind of a problem.

In conclusion I wish briefly to call especial attention to two sets of facts. The first is that in controlling human beings the "techniques" employed include such things as teaching, persuading, exhorting, or finally deception and coercion (which may presumably be practiced for "good" as well as "bad" ends). The point is that such concepts have no meaning in connection with the procedure for controlling physical objects. When these procedures are sometimes applied to the higher animals it is evident that we are treating them like human beings rather than like mechanisms.

The second fact, or set of facts, is closely related to the first, but of even wider significance. It is that as words like persuade and still more deceit and coercion imply, the moral implications of the control of human beings are decidedly dubious. There is not time to develop either of these points as they deserve. But in a society as expressly and vociferously grounded on the ideal of freedom as ours is, it should not be necessary to elaborate this second one at great length. I am astounded at the facility with which discussions on "controlling" society and individuals pass over the essential questions of who is to do the controlling and how society is to control its controllers. In the economic field specifically I wish personally to register hearty agreement with whoever it was who made the suggestion that we ought to be subsidizing schools of resisting salesmanship instead of schools of salesmanship. And similarly in the political field. It is questionable much of the time whether our so-called criminals are either less ethical or less defiant of the actual law and constitution than are the officials supposed to safeguard the one by enforcing the other. It does not seem to me very intelligent to get all excited over developing techniques for "control" without having some advance information as to who is to use them and "on" whom they are to be used. Particularly since in view of the type of people who do get into power in democracies it seems fairly certain that the scientist himself will generally be in the group the techniques are used "on" and not the group they will be used "by".

Irresistibly we are thrown back on the general philosophical problem already suggested but too large and too technical to go into here, the relation between controller and controlled, and between student and subject-matter. In the natural sciences it is taken for granted that these are wholly separate and directly opposed. It is "man" who studies and uses "nature." It is a pernicious fallacy to carry over this type of thinking

into the field where the student and subject-matter are of the same kind, and still more where they are identified. If the one-sided relationship is not preserved, we find ourselves committed to such absurdities as that when the scientist is experimenting with a piece of apparatus it is also in the same sense experimenting with him. The whole problem of control in society must be thought through in different terms. In any society which has aims and ideals, in any society which is not owned outright by an absolutely ruthless despot, "control" is a matter of mutual relationships, not of the one-sided character referred to by terms like control. Its members are controllers of nature and to be made in the highest degree controllers of themselves, not tools or pawns for some ruler.

The real problem of social control is the problem of securing agreement as to policy and as to the functions of individuals in promoting it where policy has to be social, and of securing the minimum of interference ("control") for each individual in the field of what are properly his private affairs. At no important point is this problem at all similar to that confronting an engineer or any real controller. Such "control" as is legitimate in society must be "with the consent of the controlled" which makes it a categorically different phenomenon. The only exceptions admissible are the cases of individuals proven incompetent to participate in "free" society, and even those are still to be treated as far as possible as ends in themselves or ultimately perhaps as "enemies," but in any case, never (in the modern civilized world), as means and instruments to the purposes of others, which is the position taken for granted with regard to natural objects when we talk in the scientific sense of knowledge, prediction and control.

